λ and λ' are equal, and remarks that this is no other than the determination of g in a spheroid of revolution having its axes equal to

$$k \text{ and } k\sqrt{2\cdot9414} = k \times 1\cdot7150.$$

In the other extreme case, when τ^2 is infinitely great, g is zero,

From this investigation the conclusion is arrived at, that for every given value of τ^2 there is only one value of p, and only one ellipsoid; and that to every such ellipsoid there is an appropriate value of g: and, further, that for every possible value of g there will be only one value of τ^2, and consequently only one ellipsoid susceptible of an equilibrium.

Also the reading of a paper, entitled, "Experimental Researches in Electricity." Eleventh series. By M. Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, was commenced.

December 21, 1837.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

The reading of Mr. Faraday's eleventh series of Experimental Researches in Electricity was resumed, but not concluded.

The Society then adjourned over the Christmas vacation to meet again on the 11th of January next.

January 11, 1838.

JOHN GEORGE CHILDREN, Esq., Vice-President, in the Chair.

The ballot for Bryan Donkin, Esq., was postponed in consequence of the number of Fellows required by the Statutes not being present.

The reading of a paper, entitled "Experimental Researches in Electricity," Eleventh Series, by Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry at the Royal Institution, &c., was resumed and concluded.

The object of this paper is to establish two general principles relating to the theory of electricity, which appear to be of great importance; first, that induction is in all cases the result of the actions of contiguous particles; and secondly, that different insulators have different inductive capacities.

The class of phænomena usually arranged under the head of *induction* are reducible to a general fact, the existence of which we may recognise in all electrical phænomena whatsoever; and they involve the operation of a principle having all the characters of a first, essential and fundamental law. The discovery which he had already made of the law by which electrolytes refuse to yield their elements to a current when in the solid state, though they give them forth freely when liquid, suggested to the author the extension of analogous explanations with regard to inductive action, and the possible reduction

of many dissimilar phænomena to one single comprehensive law. As the whole effect upon the electrolyte appeared to be an action of the particles when thrown into a peculiar polarized state, he was led to suspect that common induction itself is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term *induction*, never occurs except through the intermediate agency of intervening matter. He considered that a test of the correctness of his views might be obtained by tracing the course of inductive action; for if it were found to be exerted in curved lines it would naturally indicate the action of contiguous particles, and would scarcely be compatible with action at a distance. Moreover, if induction be an action of contiguous particles, and likewise the first step in electrolyzation, there seemed reason to expect some particular relation of this action to the different kinds of matter through which it is exerted; that is, something equivalent to a specific electric induction for different bodies; and the existence of such specific powers would be an irrefragable proof of the dependence of induction on the intervening particles. The failure of all attempts to produce an absolute charge of electricity of one species alone, independent of the other, first suggested to the author the notion that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.

The author describes various experiments, with the view of showing that no case ever occurs in which an absolute charge of one species of electricity can be given. His first experiments were conducted on a very large scale: an insulated cube, twelve feet in the side, consisting of a wooden frame, with wire net-work, every part of which was brought into good metallic contact by bands of tin foil, had a glass tube, containing a wire in connexion with a large electrical machine, passed through its side, so that about four feet of the tube entered within the cube and two feet remained without; but it was found impossible in any way to charge the air within this apparatus with the least portion of either electricity.

For investigating the question whether induction is an action of contiguous particles, the author employed, as an electrometer, the torsion balance of Coulomb with certain alterations and additions; and for deciding that of specific inductive capacity, a new apparatus, constructed for that express purpose. This apparatus consisted of two hollow brass spheres, of very unequal diameters, the smaller placed within the larger, and concentric with it; the interval between the two being the space through which the induction was to be effected. The apparatus had a tube in the lower part, furnished with a stop-cock, by means of which it might be connected with an air-pump or filled with any required gas. In place of the lower hemispherical shell of air, occupying the interval between the two spheres, any solid dielectric, of the same form, such as shell-lac, glass, or sulphur, might be substituted. Two of these instruments, precisely similar in every respect, were constructed, and the author ascertained that the inductive power was the same in both, by alter-

nately charging each and dividing the charge with the other, and finding that, in all cases, the charge remaining in the one, and also that received by the other, was very nearly half the original charge.

The experiments on which the author principally relies in support of the correctness of his views relative to induction being exerted in curved lines, are the following: a brass ball being laid on the top of an excited cylinder of shell-lac placed vertically, the charge which a carrier ball received when brought to different points near to the brass sphere was measured by means of the electrometer; and it was inferred, from the character of the electricity, that the charge was one by induction, and from its measure, that it proceeded in curved lines. By substituting for the brass sphere a disc of metal above the shell-lac cylinder, it was found that when the carrier ball was brought near to the middle of the disc no charge was communicated, although a sensible one was obtained at the edge of the disc, and also at a point above its centre, farther removed from the excited cylinder. Corresponding and very striking results were obtained when a brass hemisphere was placed on the top of the cylinder of lac. The charge communicated at the centre of the hemisphere was only one-third of that obtained at the edge of its periphery; but by taking it at a point at some height above the centre, and consequently much farther removed from the inducing cause, the charge was nearly equal to that of the periphery. Here, the author remarks, the induction fairly turned a corner, exhibiting both the curved lines or courses of its action, when disturbed from their rectilineal form by the shape, position and condition of the metallic hemisphere; and also a lateral tension, so to speak, of these lines on one another; all depending on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions. In the foregoing experiments the dielectric was air; but they were afterwards varied by substituting a fluid, as oil of turpentine, and likewise a few solid dielectrics, namely, shell-lac, sulphur, carbonate and borate of lead, flint-glass, and spermaceti, and with these, corresponding results were obtained. These results, the author considers, cannot but be admitted as arguments against the received theory of induction, and in favour of that which he has put forth.

In the course of these experimental researches, some effects due to conduction, which had not been anticipated, and which were similar to the residual charge in the Leyden jar, had been obtained with such bodies as glass, lac, sulphur, &c. If the inductive apparatus, fitted with a hemispherical cup of shell-lac, after having remained charged for fifteen or twenty minutes, was suddenly and perfectly discharged, and then left to itself, it would gradually recover a very sensible charge; the electricity which thus returned from an apparently latent to a sensible state being always of the same kind as that given by the charge. This return charge is attributed to an actual penetration, by conduction, of the charge to some distance within the dielectric at each of its two surfaces, and several experi-

ments are adduced in support of this view. With shell-lac and spermaceti the return charge was considerable; with glass and sulphur it was much less; but with air, no decided effect of the kind could be obtained. As this was an effect which might interfere with the results, in the method the author adopted for deciding the question of specific inductive capacity, and as time was requisite for this penetration of the charge, its influence on these results was guarded against by allowing, between the successive operations, as little time as possible for this peculiar action to arise.

The author thus states the question of specific inductive capacity which he had proposed to investigate:—Suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A, at equal distances, and uninsulated; A will then induce equally towards B and C. If in this position of the plates, some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same; or will the relation of C and B to A be altered by the difference of the dielectrics interposed between them?

The experiment of Coulomb, from which it appeared that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body, as the same body took in air, seemed to the author to be no proof of the truth of the assumption, that, under such variation of the circumstances as he had supposed, no change would occur. Entertaining these doubts as to the conclusions deducible from Coulomb's result, he had the apparatus previously described constructed, as being well adapted for this investigation. After rejecting glass, resin, wax, naphtha, oil of turpentine, and other substances, as unfit for the purpose in view, he chose shell-lac as the substance best calculated to serve as an experimental test of the question.

For the purpose of comparing the inductive capacities of shell-lac and air, a hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus, so as to nearly fill the lower half of the space between the two spheres; and their charges were divided in the manner already described; each apparatus being used in turn to receive the first charge, before its division with the other. As the two instruments were known to have equal inductive powers when air was contained in both, any deficiencies resulting from the introduction of the shell-lac would show a peculiar action in it, and, if unequivocally referable to a specific inductive influence, would establish the point in question.

The air apparatus being charged, and its disposable charge being 290°, this charge was divided between the two. After the division the charge in the lac apparatus was 113°, and in the air apparatus 114°. From this it appears, that whilst by the division the induction through the air lost 176°, that through lac gained only 113°. Assuming that this difference depends entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then the capacity for electric induction would be inversely as the respective loss and gain; and as-

suming the capacity of the air apparatus as unity, that of the shell-lac apparatus would be $\frac{176}{113}$ or 1·55.

When the shell-lac apparatus was first charged, and then the charge divided with the air apparatus, it appeared that the lac apparatus, in communicating a charge of 118°, only lost a charge of 86°. This result gives 1·37 as the capacity of the lac apparatus.

Both these results, the author considers, require a correction; the former being in excess, the latter in defect. Applying this correction, they become 1·50 and 1·47. From a mean of these and several similar experiments, it is inferred that the inductive capacity of the apparatus having the hemisphere of lac is to that with air as 1·50 to 1.

As the lac only occupied one half of the apparatus containing it, the other half being filled with air, it would follow from the foregoing result, that the inductive capacity of shell-lac is to that of air as 2 to 1.

From all these experiments and from the constancy of their results the author deems the conclusion irresistible, that shell-lac does exhibit a case of *specific inductive capacity.*

Similar experiments with flint-glass gave its capacity 1·76 times that of air. Using in like manner a hemisphere of sulphur, it appeared that the inductive capacity of that substance was rather above 2·24 times that of air, and the author considers this result with sulphur as one of the most unexceptionable.

With liquids, as oil of turpentine and naphtha, although the results are not inconsistent with the belief, that these liquids have a greater specific inductive capacity than air, yet the author does not consider the proofs as perfectly conclusive.

A most interesting class of substances, in relation to specific inductive capacity, the gases or aeriform bodies, next came under the author's review.

With atmospheric air, and likewise with pure oxygen, change of density was found to occasion no change in the inductive capacity. Nor was any change produced, either by an increase of temperature or by a variation in the hygrometric state.

The details are then given of a very elaborate series of experiments with atmospheric air, oxygen, hydrogen, nitrogen, muriatic acid, carbonic acid, sulphurous acid, sulphuretted hydrogen, and other gases, undertaken with the view of comparing them one with another under a great variety of modifications. Notwithstanding the striking contrasts of all kinds which these gases present, of property, of density, whether simple or compound, anious or cations, of high or low pressure, hot or cold, not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold among bodies having the gaseous form.

In conclusion, the author remarks, that induction appears to be essentially an action of contiguous particles, through the intermediation of which the electric force originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind and exactly equal in amount, but opposite in its direction and tendencies. Induction requires no sensible thickness in the conductors which may be used to limit its extent, for an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states, as long as the induction continues. But with regard to dielectrics, or insulating media, the results are very different; for their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever be their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of specific inductive capacities.

The author also refers to a transverse force with which the direct inductive force is accompanied. The experimental proof of the existence of such a force, in all cases of induction, is, from its bearing on the phænomena of electro-magnetism and magneto-electricity, of the highest importance; and we cannot but look forward with the greatest interest to the promised communication in which these and other phænomena relating to this subject will be reviewed.

January 18, 1838.

FRANCIS BAILY, Esq., Vice-President and Treasurer, in the Chair.

Bryan Donkin, Esq,; Sir John Jacob Hansler; the Rev. George Henry Sacheverell Johnson, M.A.; and George Richardson Porter, Esq., were severally elected Fellows of the Society.

"On the Variation of a Triple Integral." By Richard Abbott, Esq. F.R.A.S. Communicated by Benjamin Gompertz, Esq., F.R.S.

In the calculus of variations, the discovery of which has immortalized the name of Lagrange, that illustrious mathematician, by differentiating the function with respect to a new variable which enters into it, reduced the general problem of indeterminate maxima and minima to the solution of an equation depending on the variation of the given integral, whether single or multiple, and whose differential coefficient contains any number of variables, or which even depends on other integrals. The author investigates, in the present memoir, the case in which the given function is a triple integral; its variation being composed of two distinct parts, namely, a triple integral and another part, the determination of which must be sought from the limits of the triple integral.

" Explanation of the Phænomena of Intermitting Springs." By